which possessed the exceptional property of forming fresh alkaloids, when treated with certain acids in hydrochloric solution as in the preparation from it of atropine and homatropine. By acting upon secondary amines by chlorhydrines, he has succeeded in obtaining a series of bases analogous to tropine, and yielding, like it, other basic compounds, which resemble natural alkaloids in their properties and composition. For these bases, which perform the function of alcohols and amines, the author proposes the name of *alcamines*, and for the basic ethers derived from them, that of *alcameines*. That resulting from the action of phenylacetic acid, for instance, has a composition represented by the formula $C_{15}H_{21}NO_2$, and forms crystallisable salts. It is a powerful poison, acting on the respiration and the heart.

A curious relation has been discovered between theobromine, the active principle of cocoa, caffeine, the active principle of tea and coffee, and xanthine, a substance found in muscle, and largely contained in beef tea and Liebig's extract.

Dr. Fisher shows that xanthine may be converted into theobromine, and theobromine into caffein. The relation between these bodies seems to be that theobromine is dimethyl- and caffein is trimethyl-xanthine.

From the great importance of the cinchona alkaloids and their extensive use in medicine, it is exceedingly desirable that we should be able to make quinine artificially. This has not yet been done, but, with a view towards it, extensive researches are being made into the constitution of the cinchona alkaloids; and Skraup finds that all the four cinchona alkaloids—quinine, quinidine, cinchonine, and cinchonidine—when oxidised with potassium permanganate yield formic acid, and a base apparently related to phenol or carbolic acid. Other modes of oxidation exhibit a relation between quinine and cinchonine.

Cinchonine has been prepared artificially by treating a mixture of nitrobenzol, aniline, and glycerol with sulphuric acid. In its physiological properties it exhibits a certain relation to quinine, and, like it, reduces the temperature in fever and lessens or prevents putrefaction. It is said to differ from quinine in not producing giddiness, or ringing in the ears, and to have very little action on alcoholic fermentation.

An important paper by Plugge relates to the various strengths of aconitine. He finds that Petit's nitrate of aconitine has a poisonous action at least eight times greater than Meret's and 170 times greater than Friedländer's. Such differences as these between preparations bearing the same name have already led to fatal cases of poisoning—when aconitine has been prescribed medicinally; and Mr. Holmes considers that the only way to secure uniformity in the ordinary preparations of aconite is to prepare them only from plants grown in this country, and gathered while the plant is in flower.

In addition to many other interesting papers this volume contains a bibliography of chemistry, pharmacy, botany, and allied subjects.

Mémoires de la Société des Sciences Physiques et Naturelles de Bordeaux. 2^{e} Série, Tome v. 1^{ie} Cahier. (Paris, 1882.)

IN this number there are several interesting papers. We note "La Route d'Australie par le Thermomètre," with tables, by M. Hautreux; "Sur les Unités de Gauss," by M. Abria; "Le Téléphone à Bordeaux," by M. Auguste Bonel; "Notice sur les Communications Télégraphiques sous-marines," by the same; "Modification aux Machines à Force centrifuge," by M. O. de Lacolonge; "Températures et Densités de l'Eau dans l'Estuaire de la Gironde," by M. Hautreux; "Vérification expérimentale des Lois de Dalton relatives à l'Évaporation des Liquides," by M. E. Laval. M. P. Tannery contributes one of his useful critical notes, this time "Sur une Critique ancienne d'une Démonstration d'Archimède." The criticism is contained in § 36 of the fourth book of the "Collections" of Pappus (Hultsch's edition), and it charges Archimedes (in Prop. 18 of his "Spirals") with solving as a *solid* problem (*i.e.* with the aid of the conic sections) the following proposition:—*O M A* is a spiral, of which *O* is the pole, *O A* the axis, and *A C*, the tangent at *A*, meets the perpendicular to the axis through *O* in *C*, then *O C* is equal in length to the circumference described with *O A* as radius. M. Tannery supplies the gist of Archimedes' proof, and shows that Archimedes "fait appel simplement à l'intuition et au principe de continuité." Other points of interest turn up in the communication. Dr. Sigismond Günther is engaged upon an extended inquiry into the processes employed by the ancient mathematicians in the extraction of square roots, and in the course of his work has met with some interesting results. Some of these he puts forth in his paper "Sur la dépendance entre certaines méthodes d'extraction de la racine carrée et l'algorithme des fractions continués." The methods examined are those of Mollweide ("Commentationes mathematico-philologicæ tres," Lipsiæ, 1813), and of Alexeieff ("Sur l'extraction de la racine carrée d'un nombre," *Bulletin de la Soc. Math. de France*, t. vii. p. 167).

We have left to the last an article by Dr. Adolf Dux (translated from the *Pester Lloyd* for February 4, 1880), entitled "La Tombe du Savant." Bolyai was professor of mathematics and physics in the "Collège Réformée" at Maros-Vásárhely. No statue, nor marble mausoleum with sides covered with laudatory inscriptions, marks the spot where this *savant* lies; but the tomb, by its occupant's strict direction, is overshadowed by the boughs of an apple-tree, "*en souvenir des trois pommes* qui ont joué un rôle si important dans l'histoire de l'humanité, et il désignait ainsi la pomme d'Ève et celle de Pâris qui réduisirent la terre à l'esclavage, et la pomme de Newton, qui la replaça au rang des astres." Strangely enough, when Dr. Dux visited the tomb there hung on the tree just three apples, "ni plus ni moins."

Bolyai was not only a mathematician, he was also a poet: hence he had not only in his room a portrait of Gauss (with whom he had been associated at Göttingen from 1797 to 1802), but also the portraits of Shakspeare and Schiller.

In 1855 he wrote his own *Necrologe*, and survived its completion about one year. In this "Adieu" occur passages, some grave, and some humorous; of himself he writes, "S'il a été mauvais, la terre est délivrée de lui; s'il a été bon, il est délivré de la terre." He burned his poetical writings, and collected the ashes in a wooden cup, on which he wrote the following lines from Horace:—

> "Poesis
> Si paulum a summo discessit, vergit ad imum."

This is now preserved as a relic in the library of the College. Here too is shown a photograph taken after Bolyai's death. "Une noble figure, au front et au nez puissants, illuminée par la majesté de l'esprit et du sublime repos, avec de longs cheveux lisses, descendant jusqu'aux épaules. Quand on a vu ces traits, on comprend mieux le sens du nécrologe que Bolyai a lui-même écrit et suivant le texte duquel un noble pommier a été planté sur sa tombe.'

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Natural Selection and Natural Theology

I HAVE just received last week's issue of NATURE from England, and find in it some remarks by Prof. Asa Gray on an article of mine which appeared in the *Contemporary Review.* As he appears to solicit a further statement of my views, I shall supply a brief explanation of those passages in my article to which he refers.

This article, I must begin by observing, was written in reply to a criticism on my essay in the "NATURE Series" ("Scientific Evidences of Organic Evolution"), and, being intentionally limited to the ground covered by that criticism, it did not require to discuss all the points raised by Prof. Gray. But without reference to the original article, I shall now consider these points *seriatim.*

First, I am requested to state what I mean by urging that in my opinion there is no logical point of contact between natural science and natural theology. My answer is that natural science, *as such,* can only be legitimately concerned with the investigation of natural or physical causes, and that in whatever degree it presumes to pass beyond the territory of such investigation, it ceases to be natural science, and becomes ontological speculation. In other words, there is no point of logical contact between the methods and aims of natural science, as such, and the super-scientific conclusions which constitute the aim of natural theology. For it is the aim of natural theology to establish certain very definite conclusions with reference to the existence and the character of a "*causa causarum,*" which is acknowledged to be supernatural at least to the extent of being inscrutable by any of the methods possible to science. But from this sufficiently obvious position it does not follow, as my critic seems to insist, "that because natural phenomena can be reduced to laws and sequences of cause and effect, no legitimate or rational inference can be made by the human mind to a *causa causarum.*" Whether or not any such legitimate or rational inference can be made, from the data mentioned, *by the human mind* is not here the question, and therefore I decline to enter upon it. The only question before us is as to whether any such inference can be drawn by the human mind from the province of *natural science,* and I say that this question must be answered in the negative, seeing that there is, as I have just explained, an absence of logical contact between the sphere of natural science and the sphere of supernatural theology. Any inferences of the kind in question, be they legitimate or not, are drawn from the *general* order of Nature—*i.e.* from the *universal* prevalence of "laws and sequences of cause and effect"; therefore they are not really or logically strengthened by a mere enumeration of *particular* instances of such laws and sequences all similar in kind. The so-called law of causation as a whole being known, and its universality recognised, its true argumentative value to the theory of theism is not influenced by the explicit formulation of any number of its specific cases which the progress of science has been able, or may be able, to supply.

I allow, of course, that *the human mind* cannot avoid occupying itself with the most momentous of all questions—the nature of the First Cause and of its relations to the universe—or, in the words of Prof. Gray, that "such questions are inevitable"; I am only concerned with explaining why I conceive that such questions are not affected by any of the methods or results of natural science, which do not stand in any logical relation to them. And this introduces me to the next point in Prof. Gray's criticism. He says that I am inconsistent in first alleging that there is no point of logical contact between natural science and natural theology, and then proceeding to affirm that the theory of natural selection has proved destructive of the evidence of special design in organic nature. But I think this charge must have been made without due reflection; for, if a man believes that there is no logical connection between one thing and another, I do not understand why he should be deemed inconsistent merely because he endeavours to show the fictitious character of the logical connection which has been erroneously supposed to exist. Assuredly I think that "Darwin's theory need not, and legitimately should not, concern itself with natural theology"; but I also think that natural theology should not seek to obtain unreal support from natural science, and it is because natural theology has sought to do this in one conspicuous instance, and in that one instance has been as conspicuously met by Darwin's theory, that, as I explained in my article, it seemed to me desirable, both in the interests of science and of theology, henceforth clearly to recognise the logical gulf which is fixed between these two departments of human thought.

Next Prof. Gray observes, and quite correctly, that my view of the matter as a whole is fairly presented by the following sentences, which he quotes:—

"The facts of organic nature furnish no *evidence* of design of a quality other or better than any of the facts of inorganic nature." "Or, otherwise stated, there is nothing in the theory of natural selection incompatible with the theory of theism; but neither does the former theory supply *evidence* of the latter. Now this is just what the older theory of special creation did; for it would be proof positive of intelligent design if it could be shown that all species of plants and animals were created, that is, suddenly introduced into the complex conditions of their life; for it is quite inconceivable that any cause other than intelligence could be competent to adapt an organism to its environment *suddenly.*"

Prof. Gray then asks: "Is the writer of this quite sure that any cause other than intelligence could be competent to adapt existing organisms to their environment *gradually?*" My answer is but too easy. I must leave to others the happy position of being "quite sure" about anything relating to the possibilities of supernatural causation. For aught that I know, or for aught that any living man can ever know, not only all existing organisms, but all existing atoms, may have depended from all time, and for all their changes, sudden or gradual, upon "intelligence," without which they may not have been able either to have lived or to have moved, or even to have had their being. But how does this necessary ignorance on my part affect my statement that "the facts of organic nature present no *evidence* of design of a quality other or better than any of the facts of inorganic nature"? I confess I do not see how this failure in the evidence of design is made good by telling me that, for anything to the contrary of which I can be "quite sure," there *may* have been a designer. For I cannot follow my critic where he argues that the element of a supposed sudden introduction of an organism to its environment makes no difference in the evidence of its adaptations to its environment having been designed. He asks: "How is this presumption [*i.e.* that of special design] negatived or impaired by the supposition of Darwin's theory, that the ancestors were not always like the offspring, but differed from time to time in small particulars, yet so as always to be in compatible relations with their environment?" The answer is, that if we suppose the sudden or special creation of organisms in manifold adaptation to their several environments, we can conceive of no cause other than intelligence as competent to produce the adaptations, whereas, if the adaptations have been effected gradually, and *by the successive elimination of the more favourable variations by a process of natural causation,* we clearly have a totally different case to contemplate, and one which is destitute of any evidence of special design. Assuredly "*gradualness* is in nowise incompatible with design," and I do not suppose that there has ever been any one so foolish as to imagine that it is; but all the same, the progressive adaptations of structures to functions by such a purely physical cause as natural selection when once clearly revealed must destroy all special or particular *evidence* of design, even supposing such design to exist. For under this point of view it was *only* those variations which were "in compatible relations with the environment" which were able to survive. Only if it could be shown that the variations always took place exclusively in the directions required for a development of the adaptations, so as to leave no room for the operation of the physical cause in question—only then would the evidence of design as deduced from the theory of evolution be comparable with that evidence as deduced from the theory of special creation.

Towards the close of his letter, Prof. Gray seems to have anticipated this obvious rejoinder, for he says that, in order to make the purely physical explanation tenable, "it must be shown that natural selection scientifically accounts for the adaptation,"—*i.e.* as I understand, it must be shown that there is not some influence of an intelligent kind guiding the occurrence of the variations in the requisite lines, which, having been thus intelligently caused to arise, are then seized upon by natural selection. If this is Prof. Gray's meaning, he is certainly wrong in attributing it to Mr. Darwin, and I cannot see that it is a meaning of any argumentative use. For the burden of proof lies with the natural theologian to show that there *has* been some such intelligent guidance of the variations, not with the evolutionist to show cause why there *may not* have been such guidance. The evolutionist may freely admit that natural selection has probably not been the only physical cause at work, and even that the variations supplied to natural selection may not have been wholly fortuitous, but may sometimes have occurred along favourable lines as "responses of the organisms to their physical surroundings."[1] But such admissions would make no change in the logical aspect of the case; for, however many supplementary causes of this kind we may choose to imagine as possible, the evolutionist is bound to regard them as all alike in this—that they are of a physical or natural kind.

And this leads me to the core of the whole subject. Prof. Gray says:—

"What is probably meant is, that natural selection is a rival hypothesis to design, that it accounts for all adaptations in the organic world on physical principles, and so renders . . . the evidence of design from these adaptations of no other or better value than that from anything else in Nature." He then proceeds to object to this view, and says:—"If means and ends are practicable in inorganic nature at all, it is only by remote and indirect implication; while in organic nature the inference is direct and unavoidable. With what propriety, then, can it be affirmed that organic nature furnishes no other and no better evidence of underlying intelligence than inorganic nature? The evidence is certainly *other*, and to our thinking *better*."

This, I say, is the core of the whole subject. If once it is fully admitted and understood that organic nature is one with all the rest of the universe in the matter of physical causation, so that all the wonderful adaptations which we there encounter are the results of natural causes—survival of the fittest *plus* any number of other natural causes—then it appears to me, as I have said in the essay already alluded to, that all such cases of adaptation must fall into the same logical category, with reference to the question of design, as all or any other series of facts in the physical universe. For the only element of difference arises from the greater intricacy of the physical causation in the cases contemplated, rendering it more difficult to perceive the operation of the causes, at work, and therefore, as Prof. Gray truly asserts, rendering their operation more suggestive of design. But this element of difference does not really affect the question. For, *ex hypothesi*, the law of causation is everywhere and equally uniform, and for this reason the evidence of design in organic nature is certainly *not* other than it is in inorganic nature, nor, in view of the same reason, is it, to our thinking, better.

Florence, February 3 GEORGE J. ROMANES

THE letter of Prof. Asa Gray (NATURE, vol. xxvii. p. 291) contains a sentence which seems to me to contain the essence of the difference between the views of organic life, as held by the supporters of Natural Selection and Natural Theology. He says: "How is this presumption negatived or impaired by the supposition of Darwin's theory, that the ancestors were not always like the offspring, but differed from time to time in small particulars, *yet so as always to be in compatible relations to the environment?*" The italicised portion is just such a statement as "Design" would require, but cannot be held by scientific evolutionists, otherwise why are there so many extinct species? With "Design" there ought to be a perfecting of all species; whereas we know of so many which have been ruthlessly swept aside, owing to their having "differed (or owing to their not having sufficiently differed) from time to time in small particulars, yet" *not* "so as to be in compatible relations to the environment." Change is the evolutionist's view of life—change sometimes caused by the environment, sometimes beneficial, sometimes eventually detrimental: where beneficial, the species increases; where detrimental, other changes or extinction must ensue. Design would never have supplied us with a "Nature red in tooth and claw with ravine," nor would it have built up a system by the expensive and cruel mode of trial and error.

Cove Castle, Loch Long, N.B. J. B. HANNAY

Two Kinds of Stamens with Different Functions in the Same Flower

To the Melastomaceæ and Commelynaceæ mentioned in NATURE (vol. xxiv. p. 307, vol. xxvi. p. 386, and vol. xxvii. p. 30), may be added the genera *Mollia* (Tiliaceæ), *Lagerstrœmia* (Lythraceæ), and *Heteranthera* (Pontederaceæ), for having differently coloured anthers. In several species of *Mollia*, according to Darwin ("Forms of Flowers," p. 168, footnote), the longer stamens of the five outer cohorts have green pollen, whilst the shorter stamens of the five inner cohorts have yellow pollen; the stigma stands close beneath the uppermost anthers. In a *Lagerstrœmia* in my garden the six outer stamens have green pollen, and are much longer than the numerous inner ones, which have bright yellow pollen; the stigma stands on a level with the outer anthers. I have repeatedly seen bees alighting on, and gathering the pollen of, the inner anthers without noticing the outer ones.

In *Heteranthera reniformis* there is one long stamen (belonging to the outer whorl) having pale bluish pollen, and two short stamens (of the inner whorl) with bright yellow pollen. The stigma stands generally on a level with the anther of the long stamen. When the white flower opens, pistil and long stamen diverge, the pistil bending (almost without exception) to the right, and the stamen to the left; at the withering of the flower, they again approach each other, so that the stigma may be fertilised by the pollen of the long stamen. Visiting insects are attracted yet more to the yellow anthers of the two short stamens by their being placed close to a yellow spot, surrounded by a violet border, at the base of the upper petal.

Thus it may be safely assumed that in all these flowers, as well as in the above-mentioned Melastomaceæ and Commelynaceæ, fertilisation is almost exclusively effected by the pollen of the longer stamens, whilst the shorter stamens serve only to attract pollen-gathering or pollen-eating insects. It is far from surprising that the pollen of these latter stamens, though often produced in large quantity, should tend to degeneration. Darwin long ago came to this conclusion with respect to some Melastomaceæ with differently-coloured anthers, of which he had raised seedlings from pollen both of the longer and shorter stamens ("There is reason to believe that the shorter stamens are tending to abortion."—"Cross- and Self-Fertilisation," p. 298, footnote). The *Lagerstrœmia* in my garden being self-sterile, I fertilised some flowers with green, and others with yellow pollen of a different variety (or species?) growing in other gardens; both produced fruits with apparently good seeds, but only some of those from the green pollen have germinated.

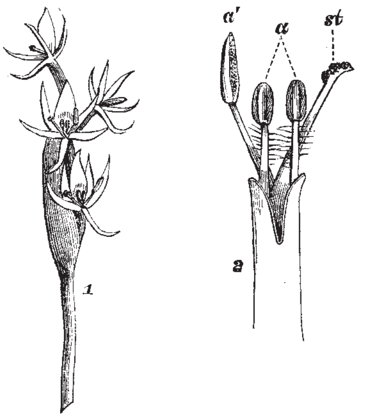

FIG. 1.—Flower-spike of *Heteranthera reniformis* (natural size). FIG. 2.—Upper end of the flower-tube, seen from behind. *a'*, the one anther of the outer whorl, with pale bluish pollen; *a*, the two anthers of the inner whorl, with bright yellow pollen; *st*, stigma.

As in all the flowers above-named, with differently-coloured anthers, the dull colour of those of the longer stamens evidently serves to make them less visible to insects, may not the green colour of the anthers of the long stamens of the mid-styled and short-styled flowers of *Lythrum salicaria* also protect them against the attacks of pollinivorous insects, to which, from protruding far from the corolla, they would be more exposed than those of the shorter stamens?

Even without being differently coloured, the stamens of the same flower may be divided into different sets with different functions. Thus in a species of *Cassia* the visiting humble-bees gather the pollen of the four intermediate stamens (the three upper ones being pollenless), which are short and straight, whilst the three lower ones are very long and curved in such a way that their pollen is deposited on the back of the humble-bees. The pistil is of the same length and curved in the same way as the longer stamens. Another very striking instance has been carefully described by Prof. J. E. Todd of Tabor (Iowa) in

---

[1] In my "NATURE Series" essay I expressly stated that natural selection is probably not the only cause of organic evolution, and therefore I think it might have been well if my critic had taken the trouble to refer to this essay before indulging in the general proposition at the close of his letter with reference to exactitude.